

Original Communications.

REMARKS ON CHOLERA.

By GORDON K. HARDIE, M.D., 73rd Regiment,
Limerick.

WHATEVER the ultimate verdict may be on the value of the eliminative treatment as compared with the stimulant, opiate, and astringent, in the early and curable stages of cholera; and though all are of one mind in condemning opiates as useless or dangerous in the later stages, Dr. G. Johnson has done well in calling attention to the futility of the stimulant treatment in the stage of collapse—unfortunately so named, as the word suggests rather the idea of syncope than of asphyxia.

But it remains for the future to decide the amount of actual harm resulting from this treatment, as contrasted with one of total abstinence from stimulants in the later stages; as not only this, but all other modes of treatment have been acknowledged equally failures, nor does Dr. G. Johnson himself seem more sanguine of success in such cases by his own mode of treatment.

Having twelve years ago witnessed a most destructive epidemic in a remote district of the Island of Mauritius, I give the following extract from published answers, shewing my views at that time (1854), on the treatment which I had adopted.

"The treatment universally adopted by me, if the case was in the early stage before symptoms of collapse and asphyxia had set in, was to administer grain doses of opium, in the form of a pill, at intervals of from half to one hour, for three or four times, combined, when possible, with stimulant aromatics and cordials. In addition, half-drachm doses of dilute sulphuric acid were given in draughts of water three or four times in the hour. In all cases seen at an early period, and in those of slight intensity, I found this treatment very effective.

"The class of cases, however, varied so much at different places, and at the same place at different periods, that this treatment was often ineffectual, and at other times inapplicable from the circumstance of commencing asphyxia having set in before I arrived. In these cases, when the state of collapse had set in, and this was the state in which I first saw by far the greater number, the sulphuric acid drinks were given without opium, and stimulant frictions with external warmth were tried.

"The result of this treatment, in the far greater number, was utterly unavailing and worthless. Wonderful recoveries, from a state bordering on death, were seen occasionally, but in such a small proportion as to make them dependent with very little probability on treatment. In short, I confess the utter powerlessness of favourably influencing the disease by treatment when in its full intensity.

"A very marked change in the type of the disease occurred at the Port of Flacq, after the most virulent form had prevailed for about ten days. The disease became much more tractable, and remedies had then a marked influence over the disease. Many who had been discouraged by the palpable failure of medicine, in the hands of the physician, vaunted, and with truth, that they had cured themselves with such simple remedies as ginger or ayaphanha tea.

"To show how impossible it is to appreciate, by apparent results only, the value of treatment, I may say, that at the Port of Flacq, of the first thirty

cases treated, I did not succeed in saving more than six, while at the estate of Queen Victoria" (where the disease appeared ten days later) "there were thirty favourable cases before one died. The measure of success depended on the early application of remedies, and the degree of the intensity of the disease.

"My experience convinces me that the time for treatment is over practically before collapse and congestion have set in; when these are once declared, I am disposed to doubt if nature, left to herself, would not produce as favourable results, as those which my experience can lay claim to.

"The douche was not tried by me, as it required more time and aid than I had at my command."

I quote from another portion of the published report of this epidemic the following.

"Another fact worthy of notice is the immunity enjoyed by the Chinese during the cholera. There were in Port Louis, in June 1854, about 1800 Chinese. The entire ascertained mortality among these 1800 men from cholera was two." (The mortality in the general population was one in twenty-four.) "This proportion, which would appear very remarkable under any circumstances, must appear wonderfully strange, when it is recollected that nine-tenths of the Chinese lived and remained in Port Louis in the very centre and hotbed of disease, and that their habits, their food, their dwellings, were of a nature to invite disease instead of repelling it. This mysterious privilege of remaining unscathed in the midst of deadly malady, amid hygienic conditions of the most unfavourable character, has been abundantly proved; but the reasons assigned are hardly satisfactory, and it is difficult to believe that the use of tea and opium* are of such miraculous preservative power."

Subsequent epidemics have hitherto not given me reason to distrust the stimulant-opiate treatment in the early stages of cholera. Nor can I hold that Dr. Johnson is justified in stigmatising it as "condemned alike by therapeutical experience and by pathological science."

The report of the Treatment Committee of the Medical Council in 1855, attributed the highest rate of success to the opiate and astringent, and the lowest to the eliminative. I see, too, that the Committee of the College of Physicians are now adhering to this plan, though the doses seem to me far too small for the purpose.

One of the latest witnesses, Mr. Davies (*Medical Times and Gazette*, May 5th), declares that, in 1849, every case of choleraic diarrhoea in which he administered castor-oil, or any purgative, died. On the other hand, Dr. Johnson, in 1854, found that "many cases of choleraic diarrhoea came under his observation, cases in which there were vomiting, bilious purging, and cramps. They were all treated by castor-oil without opiates; they all recovered, and not one case so treated passed into collapse." In this passage, as in others, Dr. Johnson speaks of choleraic diarrhoea as bilious, up to the period of collapse, thus: "This secretion" (from the bowels) "is tinged with bile before collapse comes on, and again after collapse has passed off; while during the stage of collapse it has the characteristic rice-water appearance and bile can be detected only by chemical tests." (*BRITISH MEDICAL JOURNAL*, vol. ii, 1865, p. 465.) Against this view of the facts I submit:

1. That collapse, so far as asphyxial, is not co-extensive in time with the occurrence of clear watery, or rice-watery, stools, but is a phenomenon later in date than their appearance.

* Tea = boiled water + astringents.

2. That watery or rice-watery purging in cholera epidemics is not always followed by the state of asphyxial collapse, recovery taking place in some cases without its occurrence.

3. That collapse, so far as the asphyxial symptoms are concerned, depends largely and primarily on blood changes consequent on the loss of water and salts mainly. Before going further I shall attempt an epitome of Dr. George Johnson's theory, in order that my objections to it may be more clear.

1. There is no ratio, or an inverse one, between vomiting and purging and collapse. In the worst and most malignant cases, the patient may die of collapse without either purging or vomiting having taken place. The symptoms of collapse are not such as an excessive drain of fluid is likely to produce.

2. The cholera-poison in the blood eliminates itself by purging and vomiting, and produces asphyxia directly from its irritating properties upon the smaller branches of the pulmonary arteries.

3. Consequently, astringents, by retaining the poison in the system, aggravate the disease instead of curing it.

The corner-stone of Dr. Johnson's theory is the assumed existence of a class of cases, "*the worst and most malignant*," in which the asphyxial symptoms of collapse are primary and unpreceded by vomiting and diarrhoea. Though there are numerous authorities in support of this view, I have seen two epidemics of the gravest character in Mauritius, and frequently had cases of cholera, during several years in Indian hospital practice, without ever having the fortune to find or verify one of these cases of dry cholera, and have hence grown to be very sceptical of their existence.

The physician, at a time of intense pressure, is apt to admit, without much investigation, the first statement made; and many patients, such as soldiers or sailors, are anxious to hide the self-neglect, against which they have been forewarned.

This point merits close attention in future epidemics; every alleged case should be sifted retrospectively, and *post mortem* appearances should be carefully described.

Terms suggestive of arithmetical precision are misleading in a discussion involving the intricate complexity of pathological reactions. The varied states of the recipient body are conditions of the cause, equally with the morbid poison introduced. We know too little of either sets of factors to establish ratios; but if there were an inverse ratio between watery purging and collapse, the latter should occur rather soon than late, before than after elimination; yet it will be conceded that the first evident effect of the cholera-poison is most generally diarrhoea, faecal (which, I presume, is the equivalent of bilious in Dr. Johnson's terminology) at first, later, clear or rice-watery.

The term serous, often applied to the latter, is as misleading as collapse; the one suggesting loss of the albuminous parts of the blood, just as collapse leads the mind rather to syncope than to asphyxia. Yet in cholera the loss of albuminous parts of the blood is hardly appreciable, the loss being mainly of water and salts; to this doubtless is due the frequent rapid recoveries, so different from those after hæmorrhages and other exhausting profuvia, in which the blood loses its more highly organised parts.

I believe it to be matter of fact, that asphyxial symptoms are not known to coexist with or to succeed purely faecal diarrhoea; though, when this is excessive, syncopal symptoms may ensue, producing one kind of collapse. Asphyxial symptoms, however, only follow in cases of watery or rice-watery stools, where the watery and saline parts of the blood are

diminished to such a degree as to affect the normal reactions between the blood and the air in the lungs, from which time there is diminished attraction of blood to the pulmonary capillaries, and what blood does pass through the lungs is imperfectly arterialed, the systemic arterial vessels are imperfectly filled, while the venous system and right side of the heart are filled to repletion, and the full symptoms of cholera collapse are established.

Though we cannot by any artificial experiments deprive the blood of its watery parts and salts, and hence have been driven to estimate the consequences by the analogies of blood-letting, etc., we have occasional proofs of asphyxial symptoms following copious watery purging, which has originated merely in errors of diet, or abuse of purgatives, where there has been no suspicion possible of cholera-poison. As an instance of this, I give a case in the *ipsissima verba* of a weekly report of April 30th, 1864.

"Corporal M. has had several attacks of what appears very like cholera in all its symptoms, purging of watery fluid, severe cramps of legs and arms, and a leaden look, with the cold sweats of collapse. He generally makes a rapid recovery; on this occasion, I tested the clear straw-coloured fluid passed, expecting to find it rich in albumen, but neither boiling nor the addition of nitric acid caused any precipitate.

"This may account for the slight ulterior effects of attacks apparently so severe as his have been."

Though this is a mere sketch of the case, I may say that he resembled in all essentials the asphyxial type of collapse, with death-like look, and sodden, shrivelled fingers.

This was the eighth attack, more or less severe, on record. In three of the previous attacks a red herring, a Christmas dinner, and a seidlitz powder, were the causes assigned; on this occasion, as on several others, he was unable to or unwilling to assign any cause for the attack.

I have little doubt that similar cases will be remembered by others. Their occurrence is wholly opposed to the view of the asphyxial condition being due to a spasm of the smaller branches of the pulmonary arteries, arising from the direct irritant effect of a specific poison, and lends support to the view that the central fact in cholera is a material change in the constitution of the blood, mainly a loss of water and salts, rendering it unfit to undergo the usual chemical change in the lungs (with its train of consequences on the circulation), and that this loss is mainly produced by profuse watery discharges into the gastro-intestinal canal, as evidenced by the order in time of the successive phenomena.

If the account I have given be accurate, there is good ground shown for preferring a stimulant opiate, and astringent treatment, to an eliminative one, during the short interval, when, and when only, treatment has any well proved value.

ON PROGNOSIS IN HEART-DISEASE.

By W. H. BROADBENT, M.D., Assistant-Physician to St. Mary's and the Fever Hospitals; Lecturer on Physiology at St. Mary's Hospital Medical School.

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In affections of the mitral valve, the effects of the derangement no longer fall upon the left ventricle, but on the auricle, lungs, and eventually on the right ventricle. When there is regurgitation through the orifice, it would seem, at first sight, that a certain increase of capacity would be needed to make up for this loss, and part of the force of the left ventricle will be wasted or misdirected in driving blood back-